PROCEEDINGS

OF

THE ROYAL SOCIETY.

1833–1834. No. 15.

December 5, 1833.

JOHN WILLIAM LUBBOCK, Esq., M.A., V.P. and Treasurer, in the Chair.

James Copland, M.D.; Edwin Pearson, Esq., M.A.; and Charles Terry, Esq., were elected Fellows of the Society.

December 12, 1833.

FRANCIS BAILY, Esq., Vice-President, in the Chair.

The Reports received by the Secretaries, from Sir John Herschel, Professor Airy, and Captain Smyth, on the Fluid-lens Telescope constructed for the Royal Society on Mr. Barlow's principles, were, by direction of His Royal Highness the President and Council, read to the Society at this meeting.

Sir John Herschel's Report.

I have seen Mr. Barlow's telescope at Cambridge, and examined it on several objects, in a very fine night, the 25th (if I remember) of June. As I have now no time to give it any further trial at Slough, (where I have no longer, either, any achromatic telescope of sufficient power to compare it with, all my apparatus being dismounted and in course of packing,) I will here state in few words, as *my* report on it, all I could then collect relative to its action.

1. *Achromaticity.*—Mr. Barlow's telescope is remarkably free from the dispersion of colour, very much more so than I could have expected from the nature of the correcting medium, and nearly or quite as much as could be desired.

2. *Light.*—The great aperture is very efficient under moderate powers on faint objects; and it concentrates the smaller stars well, and would, I have no doubt, show the larger nebulæ, &c., and be well available as a *sweeping telescope.*

3. *Distinctness.*—Very good with powers under 100 or 150; but on the occasion on which I tried it, it seemed to break down under high powers, and there was evidently a considerable want of correction of spherical aberration. As I had no opportunity of trying it in different temperatures, I cannot say whether this want of correction might not disappear in a different temperature,—it was about 65° when I looked through it,—neither could I ascertain whether this

arose from the glasses not being at the right distances, there being no means, or the means not having been explained to me, by which the correcting lens could be got at, to shift it.

4. A very troublesome degree of colour out of the centre of the field.

This report is of course too meagre and imperfect to conclude much from, but as both Capt. Smyth and Prof. Airy have examined it in much detail, I the less regret that my present circumstances will not allow of my going further into the subject.

J. F. W. Herschel.

July 23, 1833.

Professor Airy's Report.

From the pressure of business I have had fewer opportunities of trying the telescope than I could have desired. The absence of bright planets also has prevented me from attending so much as I wished to what I regard as the most important point in this construction, namely, the correction of colour. I have, however, had one excellent opportunity of observing the moon, and have observed several stars, single and double, and do not think that my opinion could have been altered by a greater number of observations. The correction of colour is not complete, but it is much more nearly complete than I expected, and very much more so than in a smaller telescope of Mr. Barlow's construction which I tried several years since. The colour is so far removed that it is not offensive till a power of 300 is used. But with regard to this colour, there is one point of great importance to be noticed by any person who shall try the telescope in future. It is that, in consequence of the separation of the object lenses, the only part of the field which can possibly be free from colour with a common eyepiece is in the line passing through the centres of the two object lenses; and that from the present imperfect centering, this line falls actually out of the field of the highest power (or quite on the edge). An eyepiece of a different construction and adjusted with greater care is necessary before any positive decision can be given. With regard to the definition of a star, it is not at present good, and the telescope is decidedly incompetent to separate any close star; but I regard this as a fault in the making of the surfaces, to which any telescope is liable, and which does not interfere at all in my estimation of the value of the new principle of construction. I know not how far a circumstance mentioned by Mr. Dollond (the alteration of spherical aberration with an alteration of temperature) may account for this; but so much of the irregularities are cut off by cutting off the external ring of the object glass, that I have no doubt of its being due principally to the figure.

My opinion is, therefore, that a larger telescope, as good of its kind as the present, would be very useful for nebulæ, &c.; and that if freed from defects, which do not appear to belong to the construction, it might be equal to any astronomical work except the examination of bright planets.

I have had the advantage of trying the telescope once in company with Sir John Herschel, Sir David Brewster, Mr. Cooper, Dr. Ro-

binson, and Professor Hamilton, and their opinion upon the whole coincided nearly with mine.

I beg to suggest the propriety of attaching a finder to the telescope, as much time is lost in seeking for any object.

G. B. Airy.

June 20, 1833.

Captain Smyth's Report.

I beg you will inform His Royal Highness the President, and the Council of the Royal Society, that I have this day packed up the fluid refracting telescope of which they have done me the honour of asking my opinion, and that it will be forwarded to Professor Airy without delay. It might indeed have been sent to Cambridge sooner, but that I waited for the first quarter of the present moon, to test the light and the performance of the instrument; but I regret that though I was constantly upon the spot, the weather has prevented my having an opportunity of catching her, till she was past her dichotomy, and consequently too glaring for the purpose.

On the arrival of the telescope, it was carefully unpacked, and immediately mounted, for the moment, on the lower slab of the revolving roof of my polar-axis room. It was fitted by its two pivots to the iron crutch which was sent with it, the upper parts of which were cut into Y's: the inner end was supported by Mr. Dollond's ingenious "eye-end stand." The instrument, however, was liable to tremor, both from the motion of the roof and the floor; but it enabled me to examine a few objects while poles were being prepared to form a better stage outside the observatory. And I should remark, that it was arranged with Professor Airy, who favoured me with a visit on the occasion, that my experiments were to be entirely confined to the performance of the telescope, while he would investigate its principle. My portion was to be governed by direct comparisons with my refractor, as a standard from which to assume the relative merits of the two. That instrument has a double object-glass of $5\frac{3}{10}$ths inches clear aperture, and $8\frac{1}{4}$ feet focal length; a space which I have good reason to think is accurately proportioned to the densities of the crown and flint glasses: and notwithstanding the magnitude of the diameters, the curves of the lenses seem in tolerably exact chromatic and spherical aberration throughout. It may therefore be presumed to be a more severe reference than the dimensions alone would suggest.

The temporary stage alluded to, outside the observatory, consists of two upright beams of fir, firmly driven into the bed of gravel which forms the substratum of the garden, and a cross-bar, strongly screwed, supports the iron crutch with its Y's. This is erected close to a platform and pier, which were built for some magnetic experiments, and afforded great facility in attending to the outer lens, and augmenting or diminishing its aperture. While looking towards the south, it commanded from nearly a horizontal view to above 60° of elevation; and by unshipping it, and turning it northwards, it swept the polar region. Such being the means, it remains faithfully to report what I observed, regretting, at the same time, that the weather has continued mostly unfavourable.

Monday, Feb. 25, 1833.—The evening cleared off, and was very fine from 8 till nearly 11 o'clock P.M. At 7, I placed the instrument on its stand; at 9, the thermometer was 37°·6, the barometer 29·32, and the hygrometer ·771; and the wind was at S.E.

1. *The Moon.*—The examination of the lunar cavities and shadows was rather unsatisfactory. Under the powers 250 and 400, it bore the whole aperture; but with 90 and 150, there were two spectra, one of which haunted the centre. In definition, the fluid was excelled by the flint-glass, both instruments being very steady.

2. *The great Nebula in Orion.*—This mass was seen very fairly with the whole aperture; and the trapezium was beautifully distinct under all the powers except that of 400. From the examination of this object, the best performance seems to be with the eyepieces 150 and 250. The relative light of the flint-glass and the fluid-refractors, when the latter was reduced to six inches of aperture, appeared very nearly equalized.

3. *Venus.*—This trial was altogether unsatisfactory, from the strong irradiation and the quantity of loose light. The planet was, however, low down in the west, in a stratum of mist. The only power used was the one of 90 times; but there was a great defect in distinctness.

4. *Rigel.*—This star was in the S.W., and rather low; it was therefore, as might have been expected, surrounded with teasing rays, through which I had some difficulty in detecting the small companion. The star had a spurious but broken disc, and was full of colours in every part of the field except the centre, where they were partially destroyed. Powers 150 and 250.

Tuesday, Feb. 26, 1833.—At 9 in the morning, with the thermometer at 38°·8, and the hygrometer ·798, I examined an enamelled watch-face, which is firmly fixed upon a distant chimney of solid construction; and though the solar focus could not be used, I considered sufficiently distinct vision would be obtained to test the achromaticity of the telescope. The plate itself bore the trial better than did the edges of the chimney-sides, where the focus could not be adjusted so as to prevent the alternate production of light green and purple mist, as the eye-tube was pushed in or out: and these colours scarcely disappeared, even when brought into the centre of the field of view. Some of this might probably be corrected by adjusting the fluid-lens for near objects: and Professor Barlow writes to me, that this can readily be done; but that he took off the screw-head, by which it is effected, to prevent mere lookers-on from deranging the instrument. The watch-face being upon a dark ground, I played the eye-tube till I procured a spurious disc over it, by which I was satisfied that the centering was very nearly accurate.

I then left the telescope *in statu quo*, and at half-past 12 again inspected it, the thermometer being 47°·6, the barometer 29·23, the hygrometer ·789, and the wind S.S.W.; particulars which I carefully noted, for a reason which will presently appear. The watch-plate was now considerably plainer, and its figures more sharp and distinct; but the focus required shortening in; and though there was less colour than before, I was surprised to find it verging to the prismatic extreme, and tinged with red; a circumstance which ocular foci, or the distance,

would hardly account for. I repeated the examination in the evening, when the thermometer was 45°·4, and the hygrometer ·790. I now found that the focus required lengthening; but the vision was at its best, and the colours had almost vanished, though a foggy spectrum was perceptible at times. High powers, of course, did not agree with so near an object; but they were used without greatly distorting the image.

Thursday, Feb. 28, 1833.—The weather was very fine from 10 to 12 P.M., though the wind blew occasionally in hard squalls from the S.W. The instrument appeared but little affected, and yet the observations were rendered unsatisfactory by the frequency of these gusts. At 11 the thermometer was 38°·4, the barometer 29·45, and the hygrometer ·723.

1. *σ Orionis.*—Saw 8 of the 10 stars which compose this cluster, but not sharp. The situation was unfavourable, it being two hours and a half off the meridian, and the S.W. quarter of the heavens was hazy. The power used was 250.

2. *ζ Orionis.*—This, of course, was very plainly seen; but I fished it up for its definition. The large star had a formidable nimbus, yet it did not prevent the increase of dark vacancy on raising the magnifying powers. There was much less loose light than I expected, and the small star was palpably of a pale-blue tint.

3. *Rigel.*—This star was now too far in the S.W. to be made much of: it was tremulous, and greatly irradiated under power 250. The companion was not visible, and there were two troublesome spectra.

4. *Saturn.*—The body of the planet bore magnifying powers, and showed the thin silver line of ring which now appears, without distortion, but certainly without sharp definition. I could only perceive two of the satellites, while with the flint-glass refractor I saw three. The whole aperture was too much for the instrument, and it was therefore cut off to six inches.

5. As the north was now the clearest part of the heavens, at about 11 o'clock the telescope was turned to that direction. The pole-star and its companion were seen very distinctly, even under the lowest power. This, of course, I expected; but I found that it was also viewed on both sides of the object-glass, with much less colour than the other tests I had been looking at.

Monday, March 4, 1833.—This was the best night I had yet had, and it continued very fair till nearly midnight. I was somewhat troubled with dew, but the instrument was free from tremors, and worked as well as its temporary mounting could admit of. The temperature stood thus:

	8 *o'clock.*	10 *o'clock.*	*Midnight.*
Thermometer	45°·5	43°·6	40°·5
Barometer	29·85	29·86	29·89
Hygrometer	·740	·737	·728

1. *Rigel.*—This star was greatly discoloured at the edges of the field, and was accompanied by a singular spectrum, which was not destroyed by being brought into the centre. I caught the companion

by glimpses, but it was immersed among strong rays. The powers used were 90 and 250.

2. *Sirius.*—This brilliant star was still more discoloured than Rigel on either edge of the field of view, and had a continuous production of rays, which in the centre surrounded the star, but at the sides preceded and followed it, like the wings seen where a flint-glass is not homogeneous, but fainter. These irradiations, as well as the dispersed light, were considerably cut off by diminishing the aperture of the outer lens. The powers used were 90 and 150; and I tried with 400 to raise a disc, but it was altogether too much for the object.

3. *σ Orionis.*—Saw the whole of the 10 stars of this group, but with great difficulty, and, if the term may be used, under a dim definition. Indeed, had I not practically known the object, I am doubtful whether I could have made out the middle stars. It should, however, be also stated, that it was nearly three hours to the west of the meridian.

4. *The great Nebula in Orion.*—I placed the whole aperture upon this object; and though the moon was nearly at full, I easily made out its outline, as well as that of its companion. But the trapezium of stars, under high powers, was more distinct with an aperture of 6 inches than when the whole was applied. I could make out only four stars in this spot; it will be recollected, however, it was now three hours past the meridian, for the time of its transit will not allow of earlier experiment. This I regret, because so fine a constellation, from its composition and place, offers in itself a *thesaurus* of astronomical tests.

5. *γ Leonis.*—This beautiful double star was remarkably well seen, being nearly on the meridian. There was, however, much false light, but it did not hinder the colours being seen: the large star was slightly red, and the small one a Saxon green. The powers used were 90, 150, and 250.

6. *ω² Leonis.*—This was a test which, in the deficient arrangement of the apparatus, I could not manage; but notwithstanding there was much dispersed light, I should pronounce that with power 400 I saw the star elongated, and different from the other two *omegas*.

7. *The Præsepe.*—An examination of this cluster was very favourable to the defining power of the telescope, and its general distribution of light. I tried it under the eyepieces 90, 150, and 250.

8. *ζ Cancri.*—With some difficulty I made out this object to be triple, under a power of 250: that of 400 broke the rings of the spurious discs with disagreeable rays, so as to confuse the whole vision.

9. *Saturn.*—The planet was about two hours and a half to the east of the meridian when I placed the telescope upon it. It was tolerably defined, but with *muddy* edges, though it bore magnifying pretty fairly. I saw two satellites steadily, and a third by glimpses; and this was all I could do with my own telescope at the time of transit. The ring resembled a thin silvery bar lying equatorially across the planetary disc, and was sharper than the body of Saturn.

Wednesday, March 20, 1833.—I had now intended to wait for the first quarter of the new moon; but the night proved so fine and dark, that I re-examined some of the former tests, and observed some new

ones. There was a light N.W. wind, and the temperature was thus:

	9 *o'clock.*	*Midnight.*
Thermometer	37°·7	34°·4
Barometer	30·01	30·00
Hygrometer	·680	·670

1. *The great Nebula in Orion.*—This was now three hours and a quarter over the meridian, and yet it was seen in great beauty and distinctness under the whole aperture, with eyepieces 90 and 150. The trapezium was examined very closely with 250 and 400, which last it bore better than it had yet done; but only four stars were visible.

2. *σ Orionis.*—All the stars of this group were perceptible under the power 250, but they had the appearance of being seen in a second-rate reflector; so that I know not how a micrometer would work upon this instrument.

3. *Venus.*—The crescent which this planet now forms was better seen than heretofore, but an unseemly quantity of light still attended it; and under the higher powers the colours were intolerable. When, however, the focus of power 90 was nicely adjusted, and the planet brought exactly into the centre of the field, it was a beautiful object, despite of a secondary spectrum. The aperture was reduced, and I did not find, either now or on other occasions, that this sensibly affected the ocular focus.

4. *γ Leonis.*—This brilliant object was distinctly seen, and the dark vacancy between the stars increased more than did the spurious discs, while the magnifying powers were being raised, though much loose light and irradiations were thereby produced. And it is singular that the separation was improved by my placing a central disc of card-paper, two inches in diameter, on the outer lens.

5. *Messier's 46th Nebula.*—This was very fairly resolved into stars, and better with the whole than the reduced aperture. Eyepiece 90 showed it easily, but the higher powers gave it a very *turbid* appearance. The preceding cluster was brilliant.

6. *α Leonis.*—This star had a bunch of disagreeable rays shooting from it; and the light, when under the best adjustment I could give the focus, was curiously thrown to the northward. I was able, however, to raise a tolerable disc, and the small star at a little distance from Regulus was unusually distinct.

7. 24 *Comæ Beren.*—I pointed to this remarkably pretty object to test the colours, and very readily perceived the large star to be of a bright orange colour, and the small a sea-green. This was one of the best sights I had yet had, and on the whole was satisfactory.

8. *ι Leonis.*—This, though a very close and unequal double star, was well shown, yet at times the stray light would obscure the companion. The large star was fairly figured, and the small seemed about the 10th magnitude, and of a greenish hue. It formed a fine test.

9. *Saturn.*—I had a good trial of this planet; and though the powers 90, 150 and 250 were borne, the disc was certainly not well

defined. The ring is still a mere *bar* lying across the equator: it was very well shown, as were also three satellites. When I applied the power 400, the whole field was strewed with harsh light.

10. *γ Virginis.*—This interesting star, though now so exceedingly close, was made double with 250, and very well shown; but with 400 there was great tremor and irradiation, so that the discs were often confused into one.

Saturday, March 30, 1833.—After a continuance of bad weather for several days, it cleared off a little; but in the mean time I had missed the favourable phase of the moon, for which I had been waiting. I therefore closed my examinations with the following one:

The Sun.—From the extreme volatility of the sulphuret of carbon, I was fearful of its expansion, and therefore had not yet turned the telescope upon the sun, lest the condensation of the solar rays, at the place where they traverse the fluid, should prove too much for the lenses. But on mentioning this apprehension to Professor Barlow, that gentleman assured me that an exposure of from five to ten minutes could do no mischief. I therefore this day reduced the aperture to three inches, and directed the instrument to the solar disc, when, sweeping over the luminary for about three minutes, I found the surface was quite clear of spots. On turning from it, I drew out the eye-tube, and looking at the fluid, perceived that the bubble was considerably diminished, but not so much as I had expected. This was the only time that I exposed the telescope to great heat.

These are the only experiments I have been able to make; and the season of the year, together with the inefficiency of the apparatus, have certainly prevented me from assigning exact limits to the performance of this telescope. Still, as I had immediate reference to one of the best refractors extant, I may add the following conclusions, premising, that I have not constantly noted down the performance of the latter upon each test, because my end was to pronounce upon the fluid object-glass. I should also observe, that the magnifying powers of both the instruments were equally matched, and their apertures were generally proportioned to nearly six inches: the eyepieces were thus:

Fluid refractor	90	150	250	400
Flint-glass ditto	93	157	240	416

From the result of my observations, it has struck me that this ingenious principle has strong claims to consideration for its valuable optical powers, but that, in the present stage, it is more adapted for stars than for planets; and should the application of it be tried on a larger scale, it might be made with sufficient illumination to examine the high-class nebulæ; a branch of practical astronomy which is now nearly shut against refractors. The defining power does not strike me as being so good as the light, nor does the achromatism seem to be perfect. Yet I should mention the want of focal and mechanical arrangement; that the only adjustment I had for distinct vision was by the hand, with the sliding eyepiece tube; and that slight derangements might be occasioned by the mounting and dismounting of the great tube, however carefully it was attended to.

I cannot but suspect that the performance of this telescope is affected by temperature, and that severe tests in the summer months might afford different conclusions to those which I have arrived at; but as I considered my opinion was desired on the instrument in its present state, I took no means for applying artificial heat. And, perhaps, the secondary spectrum which haunts the field might be mitigated, and the prismatic colours destroyed, by an alteration of the distance between the fluid and outer lenses; but the same consideration prevented my applying for a screw, by which it might have been effected.

But there is one condition of the instrument which, if correct, would be of greater importance than the rest, as connected with this Report. It strikes me forcibly, from the several effects I observed, that the focus has been cut too short; a defect which would seriously affect the spherical aberration of the outer or object lens and its dispersion: and this would account for the fluid refractor not performing better than the flint-glass one, without impugning the corrective powers of the sulphuret of carbon, or its skilful application by the scientific Professor.

April 4, 1833. W. H. Smyth.

A paper was then read, entitled, "An Account of some Experiments made in the West Indies and North America, to determine the relative Magnetic Forces, in the years 1831, 32, and 33." By the Rev. George Fisher, M.A., F.R.S.

The experiments of which the results are given in this paper were made by Mr. James Napier, late Master of H. M. S. Winchester. The needles were precisely similar to those used in the experiments described by the author in a former paper; and the observations were made with great care, and repeated several times at the same places; by which it appeared that the intensities of the needles continued unchanged during the whole period of the experiments; and the mean of all those made at one place was taken as the result. From these the relative forces at different places were computed, and stated in the form of a table.

A paper was also read, entitled, "On the Theory of the Moon." By John William Lubbock, Esq., V.P. and Treas. R.S.

M. Poisson, in a memoir which he has lately published on the Theory of the Moon, expresses the three coordinates of her path, namely, her true longitude, her distances, and her true latitude, in terms of the time. The author observes that the reasons for so doing adduced by M. Poisson, are the same as those which led Mr. Lubbock also to deviate from the course which had previously been always pursued by mathematicians, and to employ equations in which the true longitude is the independent variable. Instead, however, of integrating the equations of motion by the method of indeterminate coefficients, as the author had proposed, M. Poisson recommends the adoption of the method of the variation of the elliptic constants. In the present paper, Mr. Lubbock states the reasons which have determined him not to employ the latter method, founded chiefly on the advantages of obtaining complete uniformity in the methods used in the theories

of the moon and of the planets, and also in that of a greater rapidity of approximation by the improvements introduced in these methods.

Laplace, in the Mécanique Céleste, alludes to an equation of long period, of which the argument is twice the longitude of the moon's node, plus the longitude of her perigee, minus three times the longitude of the sun's perigee; and M. Poisson has shown that the coefficient of the corresponding argument in the development of the disturbing function equals zero: but the author shows that the same result may be arrived at very simply, by means of the method of developing the variation of the disturbing function.

December 19, 1833.

MARK ISAMBARD BRUNEL, Esq., Vice-President, in the Chair.

A paper was read, entitled, "On the Position of the North Magnetic Pole." By Commander James Clark Ross, R.N., F.R.S.

The author remarks that the discordances in former observations made with a view to determine the position of the magnetic pole, have arisen partly from the irregularity of distribution in the earth of the substances which exert magnetic power, and partly from the great distances from the magnetic poles at which these observations have been made. The latter cause of uncertainty has been now, in a great measure, removed, by the numerous and accurate observations made during the late arctic expeditions. The object of the present paper is to put on record those which were made in the last voyage of Captain Ross, in which a spot was reached corresponding to the true north magnetic pole on the surface of the earth. The nature of the instruments, and the difficulties encountered in their practical employment, under the circumstances of the expedition, are fully stated. Having arrived, on the 1st of June, at north latitude 70° 5′ 17″, and west longitude 96° 45′ 48″, the horizontal magnetic needle exhibited no determinate directive tendency, and the dipping needle was within a minute of the vertical position, a quantity which may be supposed to come within the limits of the errors of observation; hence the author concludes that this spot may be considered as the true magnetic pole, or as a very near approximation to it, as far, at least, as could be ascertained with the limited means of determination of which he was then in possession.

A table of the observations, including those on the intensity of the magnetic force at various stations, is subjoined.

A paper was also read, entitled, "On the Quantity and Quality of the Gases disengaged from the Thermal Spring which supplies the King's Bath, in the City of Bath." By Charles G. B. Daubeny, M.D., F.R.S., Professor of Chemistry in the University of Oxford.

The author, pursuant to an intention expressed in a former paper read to the Society, undertook a series of experiments, for the purpose of measuring the gas evolved from the thermal springs at Bath during

a period of time sufficiently long to enable him to determine with tolerable precision its average amount, and to ascertain whether any great diurnal variations in its quantity can be detected. He also kept during the same period a corresponding register of the conditions of the atmosphere, as to temperature, humidity and pressure, in order to learn whether any connexion could be traced between these conditions and the quantities of gas evolved. The supplies, both of water and of gas, from the Hot Bath and the Cross Bath being insignificant compared with those from the King's Bath, the author confined his inquiries to the last of these, and chiefly to the gas arising from the apertures within its central area, which is about twenty feet in diameter; the other apertures without this circle from which gas issued being carefully stopped up. The gas was collected by a funnel-shaped apparatus, constructed of several sheets of iron riveted together, and the seams rendered airtight by white lead, supported on a frame, with contrivances for raising and lowering it as occasion might require. The observations were made during periods of from five to fifteen minutes, and continued daily from the 17th of September to the 18th of October inclusive. The average quantity of gas evolved per minute, as deduced from the mean of all the observations, is 267 cubic inches, giving a total daily volume of 223 cubic feet.

The author, by referring to the accounts on record of other thermal waters, concludes that the evolution of gas is a phenomenon as intimately connected with the constitution of these waters, as the presence of a definite quantity of certain saline ingredients, or the possession of a particular temperature; both of which probably continue unaltered for periods of indefinite duration, compared with the records of any human history. He considers this phenomenon to be explicable, by supposing that a large volume of these gases is pent up in some cavern of rock, at a great depth below the surface of the earth, which, at some former period, had been heated by volcanic action, and which, by the gradual cooling and consequent contraction of its external portions, exerts a continued pressure on the gaseous contents of its cavity, and determines the uniform flow of a stream of gas through crevices towards the surface.

It appears from the observations of the author that the quantities of gas disengaged, in a given time, from the King's Bath are somewhat variable; for the differences between the results obtained on successive days are too considerable to be ascribed either to errors of manipulation or to variations in the amount of gas escaping by other avenues. These fluctuations in quantity cannot be traced to have any connexion with those of the atmospheric pressure. Variations likewise were observed in the proportional quantities of carbonic acid contained in the gas evolved at different times, which latter variations the author thinks may perhaps be dependent on the former.

The author remarks, in conclusion, that the immensity of the volume of nitrogen gas which is disengaged from these thermal springs, and the entire absence of carburetted, sulphuretted and phosphuretted hydrogen, seems to afford additional presumption against the truth of the opinion that the nitrogen gas which escapes from volcanoes

and from these springs is derived from atmospheric air, held in solution by the water, and deprived of the greater part of its oxygen by animal and vegetable putrefaction. He is disposed to ascribe the deficiency of oxygen to some process of combustion, during which it unites with some base, forming a compound not easily volatilized by heat; and to account for the presence of carbonic acid, by the calcination of earthy carbonates, rather than by the combustion of coal or bitumen.

The Society then adjourned over the Christmas Vacation, to meet again on the 9th of January.

January 9, 1834.

BENJAMIN COLLINS BRODIE, Esq., Vice-President, in the Chair.

The Earl of Tyrconnel was elected a Fellow of the Society.

A paper was read, entitled, "On the empirical Laws of the Tides in the Port of London, with some Reflections on the Theory." By the Rev. William Whewell, M.A., F.R.S., Fellow and Tutor of Trinity College, Cambridge.

The present state of our knowledge of the tides is represented by the author as extremely imperfect, and at variance with the scientific character which Physical Astronomy is supposed to have attained; for although it be the universally received opinion that they are the direct results of the law of gravitation, the exact laws by which the phenomena are actually regulated with regard to time and place have never been strictly deduced from this general principle. The tide tables that have been given to the world are calculated by empirical methods, which are frequently kept secret by those who employ them; and the mathematical solutions of the problem hitherto attempted have been confessedly founded on hypotheses which are in reality very remote from the real facts; and accordingly it is doubtful whether they give even an approximation to the true result. The comparison of the results of theory with extensive series of observations had not been attempted previously to Mr. Lubbock's discussion of the tides of the port of London, recorded in the Philosophical Transactions for 1831. The establishment, on theoretical grounds, of rules for the calculation of tide tables, has been attempted by Bernoulli and by Laplace: the methods recommended by the former are probably the foundation of those at present used by the calculators of such tables, that of Laplace being complicated, and too laborious for practice. Original tide tables are very few; none, with which the author is acquainted, deserving that title, except those which are published for Liverpool, and those for London. The former, which are calculated according to rules obtained from Mr. Holden, from the examination of five years of observations, made at the Liverpool docks by Mr. Hutchinson, at that time harbour-master, are remarkably

correct. Several tide tables for London are annually published; but they vary considerably from one another. The method generally practised in England for the construction of tide tables for other places, has been to add or subtract some constant quantity, according to the place, assuming as a basis the tide tables either of London or of Liverpool; but this assumption of a constant difference is shown by the author to be, in various instances, incorrect. Much, therefore, remains to be done, before we can hope to arrive at a scientific solution of this problem.

The author then proceeds to examine the empirical laws of the tides of the port of London, deducible from the records of the nineteen years of observations which have been discussed by Mr. Dessiou, under the direction of Mr. Lubbock, and which include 13,073 observations. His first object is to determine the manner in which the time of high-water is affected by the following conditions, namely, the right ascensions, declinations and parallaxes of the sun and moon; for which purpose he considers at some length, first, the establishment; secondly, the semimenstrual inequality; thirdly, the corrections for lunar parallax; fourthly, the lunar declination; and lastly, the solar parallax and declination. He next discusses the empirical laws of the height of high-water; which he observes will be affected in the same manner as the periods of the tides, by a semimenstrual inequality, by corrections for lunar parallax and declination, and by a solar correction; and concludes by giving a formula for computation which comprehends all these elements. He then enters into a comparison of the results thus obtained with the theory of Daniel Bernoulli, according to which the waters of the ocean assume nearly the form in which they would be in equilibrium under the actions of the sun and moon, on the supposition that the pole of the fluid spheroid follows the pole of the spheroid of equilibrium at a certain angular distance; and that the equilibrium corresponds to the configuration of the sun and moon, not at the moment of the tide, but at a previous moment, at which the right ascension of the moon was less by a constant quantity. The author thinks, however, that it would not be safe to attempt to deduce from the preceding investigations any general views concerning the laws of the tides, for it is not likely that the discussion of observations at any one place should exhibit clearly the true principles of the theory, especially as, in the present case, it so happens that the phenomena of the tides at London are in some measure masked by a curious combination of circumstances, namely, by the mouth of its river being on the side of an island, turned away from that on which the tide comes, and so situated that the path of the tide round one end of the island is just twelve hours longer than round the other.

In consequence of the time required to transmit to any port the general effect of the tide-producing forces being different from the time required to transmit to the same port the effects of particular changes in these forces; or, in other words, from the epochs of the changes due to parallax and declination being different from the epoch of the semimenstrual inequality, it follows that although the general

form of the terms, and the variable part of the arcs on which they depend, may be deduced from the theory of equilibrium, yet the constant epoch which occurs in each of these arcs, and which determines when the inequality vanishes, and reaches its maximum, will probably have to be determined, in all cases, by observation.

In conclusion, the author gives a statement of what appears to him to be the most important steps from which any great improvement to our knowledge on the subject of the tides may be hoped; and recommends the discussion of extensive collections of observations made at a variety of places, in a manner similar to what has been done by Mr. Dessiou with regard to those at London; and the comparison with one another of the empirical laws resulting from their separate investigation. Very valuable materials for this purpose, he expects, will hereafter be furnished by the observations now making, on a judicious system, at the St. Katharine's docks.

January 16, 1834.

JOHN WILLIAM LUBBOCK, Esq., M.A., V.P. and Treasurer, in the Chair.

A paper was read, entitled, "On a new property of the Arcs of the Equilateral Hyperbola." By Henry Fox Talbot, Esq., M.P., F.R.S.

By an analytical process, the author arrives at the following theorem, namely, if three abscissæ of an equilateral hyperbola be materially dependent by reason of two assumed equations, which are symmetrical with respect to these three abscissæ, the sum of the arcs subtended by them is equal to three quarters of the product of the same abscissæ, or only differs therefrom by a constant quantity. In order to satisfy himself of the correctness of this theorem, the author calculated various numerical examples, which entirely confirmed it. This simple result is essentially a relation between three arcs of the equilateral hyperbola, and is by no means reducible to a relation between two; and therefore is not reducible to the celebrated theorem of Fagnani, concerning the difference of two arcs of an ellipse or hyperbola, nor to any other known property of the curve.

The reading of Mr. Faraday's Sixth Series of Experimental Researches in Electricity was commenced.

January 23, 1834.

FRANCIS BAILY, Esq., Vice-President, in the Chair.

A paper was read, entitled, "Appendix to a Memoir, lately read to the Society, on the Quality and Quantity of the Gases disengaged from the Hot Spring of the King's Bath, in the City of Bath." By Charles G. B. Daubeny, M.D., F.R.S.

The author has lately examined two tepid springs, which, since the setting in of the wet weather, have broken out at the foot of St.

Vincent's rocks, Clifton, immediately below the Cliff, against which the suspension bridge over the Avon is designed to abut. The temperatures of the springs were 72° and 66° respectively; and the gas consisted of 92 parts of nitrogen, eight of oxygen, and three of carbonic acid. The author deduces from these facts arguments in confirmation of the views he has stated in the paper to which this is an appendix.

Mr. Faraday's Sixth Series of Experimental Researches in Electricity were resumed and concluded; and the reading of the Seventh Series commenced.

The Society then adjourned over the following Thursday, being the Day of the Martyrdom of King Charles the First, to meet again on the 6th of February.

February 6, 1834.

JOHN WILLIAM LUBBOCK, Esq., M.A., V.P. and Treasurer, in the Chair.

Captain Chesney, Roy. Art.; Thomas Copeland, Esq.; the Right Hon. Sir Edward Cust, K.C.B.; James Horne, Esq.; John Russell Reeves, Esq.; Lieut.-Col. William Henry Sykes, E.I.C.S.; and John Waterhouse, Esq., were elected Fellows of the Society.

The reading of Mr. Faraday's Seventh Series of Experimental Researches in Electricity was resumed in continuation.

February 13, 1833.

HIS ROYAL HIGHNESS THE DUKE OF SUSSEX, K.G., President, in the Chair.

The reading of Mr. Faraday's papers was resumed and concluded.

"Experimental Researches in Electricity.—Sixth and Seventh Series." By Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry in the Royal Institution of Great Britain.

In the course of his experimental investigation of a general and important law of electro-chemical action, which required the accurate measurement of the gases evolved during the decomposition of water and other substances, the author was led to the detection of a curious effect, which had never been previously noticed, and of which the knowledge, had he before possessed it, would have prevented many of the errors and inconsistencies occurring in the conclusions he at first deduced from his earlier experiments. The phenomenon observed was the gradual recombination of elements which had been previously separated from each other by voltaic action. This happened when, after water had been decomposed by voltaic electricity, the mixed gases resulting from such decomposition were left in con-

tact with the platina wires or plates, which had acted as poles; for under these circumstances they gradually diminished in volume, water was reproduced, and at last the whole of the gases disappeared. On inquiring into the cause of this reunion of the elements of water, the author found that it was occasioned principally by the action of the piece of platina, which had served for the positive pole; and also that the same piece of platina would produce a similar effect on a mixture of oxygen and hydrogen gases obtained by other and more ordinary kinds of chemical action. By closer examination, it was ascertained that the platina, which had been the negative pole, could produce the same effect. Finally, it was found that the only condition requisite for rendering the pieces of platina effective in this recombination of oxygen and hydrogen is their being perfectly clean, and that ordinary mechanical processes of cleaning are quite sufficient for bringing them into that condition, without the use of the battery. Plates of platina, cleaned by means of a cork, with a little emery and water, or dilute sulphuric acid, were rendered very active; but they acquired the greatest power when first heated in a strong solution of caustic alkali, then dipped in water to wash off the alkali, next dipped in hot strong oil of vitriol, and finally left for ten or fifteen minutes in distilled water. Plates thus prepared, placed in tubes containing mixtures of oxygen and hydrogen gases, determined the gradual combination of their elements: the effect was at first slow, but became by degrees more rapid; and heat was evolved to such a degree, indeed, as frequently to give rise to ignition and explosion.

The author regards this phenomenon as of the same kind as that discovered by Davy in the glowing platina; that observed by Döbereiner in spongy platina, acting on a jet of hydrogen gas in atmospheric air; and those so well experimented on by MM. Dulong and Thenard. In discussing the theory of these remarkable effects, the author advances some new views of the conditions of elasticity at the exterior of a mass of gaseous matter confined by solid surfaces. The elasticity of gases he considers as being dependent on the mutual action of the particles, especially of those which are contiguous to each other; but this reciprocity of condition is wanting on the sides of the exterior particles which are next to the solid substance. Then, reasoning on the principle established by Dalton, that the particles of different gases are indifferent to one another, so that those of one gas may come within almost any distance of those of another gas, whatever may be the respective degrees of tension in each gas among the particles of its own kind, he concludes that the particles of a gas, or of a mixture of gases, which are next to the platina, or other solid body not of their own chemical nature, touch that surface by a contact as close as that by which the particles of a solid or liquid body touch each other. This proximity, together with the absence of any mutual relation of the gaseous particles to particles of their own kind, combined also with the direct attractive force exerted by the platina, or other solid body, on the particles of the gases, is sufficient, in the opinion of the author, to supply what is wanting in order to render effective the affinity between the particles of oxygen and hydrogen; being, in

fact, equivalent to an increase of temperature, to solution, or to any of the other circumstances which are known to be capable of adding to the force of the affinities inherent in the substances themselves.

Some very curious cases of interference with this action of platina and other metals are next described. Thus, small quantities of carbonic oxide, or olefiant gas, mixed with the oxygen and hydrogen gases, totally prevent the effect in question; while very large quantities of carbonic acid, or nitrous oxide gas, do not prevent it: and it is remarkable, that the former of these gases do not affect the metallic plates permanently; for if the plates be removed from those mixtures, and put into pure oxygen and hydrogen gases, the combination of these elements takes place.

The author concludes by some general notice of numerous cases of physical action, which show the influence of certain modifications of the conditions of elasticity at the external surface of gaseous bodies.

The seventh series, which is a continuation of the subject of the fifth, namely, electro-chemical decomposition, commences with a preliminary exposition of the reasons which have induced the author to introduce into this department of science several new terms, which appear to be required in order to avoid errors and inaccuracies in the statement both of facts and theories. As a substitute for the term *pole*, and with a view to express also a part of the voltaic apparatus to which that name has never been applied, although it be identical with a pole in its relation to the current, the author proposes to employ the term *electrode*. The surfaces of the decomposing body, at which the positive current of electricity enters and passes out, are denominated respectively the *eisode* and the *exode*. Bodies which are decomposable by the electric current are called *electrolytes*, and when *electro-chemically decomposed*, they are said to be *electrolyzed*; the substances themselves, which are evolved in such cases, being called *zetodes*, and the terms *zeteisode* and *zetexode* being applied, accordingly as the substance passes in one direction or the other. The propriety and the advantage of employing these new terms, the author observes, can be properly appreciated only by an experience of their uses and applications in the exposition of the theory of decomposition given in the fifth series of these inquiries, and of that of definite electro-chemical action advanced and supported in the present paper.

The first section of this paper is occupied with the consideration of some general conditions of electro-chemical decomposition. It has been remarked, that the elements which are strongly opposed to each other in their chemical affinities are those most readily separated by the voltaic pile; and the discovery of the law of conduction, explained in the fourth series, has led to a great augmentation of the number of instances which are in conformity with this general observation: but it is here shown, that the proportion in which the elements of a body combine has great influence on the electro-chemical character of the resulting substance; and that numerous instances occur where, although one particular compound of two substances is decomposable, another is not. It appears, that whenever binary compounds of simple

bodies are thus related to one another, it is the proto-compounds, or those containing single proportions, which are decomposable, and that the per-compounds are not so.

The second section contains an account of a new instrument devised by the author, for exactly measuring electric currents, and which he terms the *volta-electrometer*. The current to be measured is made to pass through water acidulated by sulphuric acid, and the gases evolved by its decomposition are collected and measured, thereby giving at once an expression of the quantity of electricity which has passed. The principle on which this conclusion is founded is the new law discovered by the author, "*that the decomposing action of any current of electricity is constant for a constant quantity of electricity.*" The accuracy of this law was put to the test in every possible way, with regard to the decomposition of water, by making the same current pass in succession through two or more portions of water, under very different circumstances: but whatever were the variations made, whether by altering the size of the poles or electrodes, by increasing or lessening the intensity of the current or the strength of the solution, by varying its temperature or the mutual distance between the poles, or by introducing any other change in the circumstances of the experiment, still the effect was found to be the same; and a given quantity of electricity, whether passed in one or in many portions, invariably decomposed the same quantity of water. No doubt, therefore, remains as to the truth of the principle on which the volta-electrometer acts: but with regard to the practical application of the principle, several forms of the instrument are described by the author, and the mode of employing them, either as the measurers of absolute quantities, or as standards of comparison, are fully pointed out.

In the third section of the paper, the primary or secondary character of the bodies evolved at the electrodes is discussed. It is shown that they are secondary in a far greater number of cases than has usually been imagined; and that laws have been deduced with regard to the ultimate places of substances, from the appearance of the secondary products; so that certain conclusions, true in themselves, have hitherto been obtained by erroneous reasoning, since the facts which were supposed to support them have, in truth, no direct relation with those conclusions. The methods of distinguishing primary and secondary results from each other are explained, and the importance of this distinction towards the establishment of the law of definite electro-chemical action is insisted upon by the author.

The fourth section is entitled, "On the definite Nature and Extent of Electro chemical Decomposition," and is considered by the author as by far the most important of this or indeed of the whole series of investigations of which he has now presented the results to the Royal Society. He adverts to the previous occasions on which he has already announced, more or less distinctly, this law of chemical action; and also to the instrument just explained as one of the examples of the principle about to be developed. He next refers to experiments described in another part, in which primary and secondary results are distinguished as establishing the same principle with regard to muriatic

acid; the results showing, that not only the quantity of that acid decomposed is constant for a constant quantity of electricity, but that, when it is compared with water, by making one current of electricity pass through both substances, the quantities of each that are decomposed are very exactly the respective chemical equivalents of those bodies. The same current, for example, which can decompose nine parts by weight of water, can decompose thirty-seven parts by weight of muriatic acid, these numbers being respectively the chemical equivalents of those substances, as deduced from the phenomena of ordinary chemical action.

Cases of decomposition are then produced, in which bodies rendered fluid by heat, as oxides, chlorides, iodides, &c., are decomposed by the electric current, but still in conformity with the law of constancy of chemical action. Thus the current which could decompose an equivalent of water, could also decompose equivalents of muriatic acid, of proto-chloride of tin, of iodide of lead, of oxide of lead, and of many other bodies, notwithstanding the greatest differences in their temperature, in the size of the poles, and in other circumstances; and even changes in the chemical nature of the poles or electrodes, and in their affinities for the evolved bodies, occasioned no change in the quantity of the body decomposed.

The author proceeds, in the last place, to consider a very important question with relation to chemical affinity, and the whole theory of electro-chemical action, namely, the absolute quantity of electricity associated with the particles or atoms of matter. This quantity he considers as precisely the same with that which is required to separate them from their combination with other particles when subjected to electrolytic action, and he brings many experiments to bear upon this point; describing one, in particular, in which the chemical action of 32·5 parts of zinc, arranged as a voltaic battery, was able to evolve a current of electricity capable of decomposing and transferring the elements of 9 grains of water, being the full equivalent of that number. The relation of electricity, thus evolved, to that of the common electric machine is pointed out in a general way, and the enormous superiority as to quantity, in the former mode of action, is insisted upon. In conclusion, the author refers to a statement which he has made in the third series of these researches, in which he expresses his belief that the magnetic action of a given quantity of electricity is also definite; and he is now more confident than ever that this view will be fully confirmed by future experiment.

The reading of a paper, entitled, "An Inquiry into the Nature of Death; being an attempt to ascertain its more immediate causes, with a view to the better regulation of the means of obviating them." By A. P. W. Philip, M.D., F.R.S. L. & Ed.—was commenced.

February 20, 1834.

HIS ROYAL HIGHNESS THE DUKE OF SUSSEX, K.G., President, in the Chair.

The reading of Dr. Philip's paper was resumed and concluded.

The object of the present paper, which the author intends as a sequel to those he has lately presented to the Society, and which have been published in the Philosophical Transactions, is to investigate the operation of the different causes of death, and the mode in which the several powers of the living system influence each other during the period of their decline. In the more perfect animals, he observes, there are three distinct classes of functions, namely, the sensorial, the nervous and the muscular, which have no direct dependence on each other, although they are linked together by the connexions of the organs in which they reside; the consequence of which is, that the cessation of any one class of functions is more or less immediately followed by the destruction of the rest. What is commonly called *death* consists in the extinction of the sensorial functions only; for the nervous and muscular functions may still, for a time, survive; although, in consequence of the failure of respiration, which in the more perfect animals the author considers as, in the strictest sense, a function of volition, they also speedily terminate. Thus he distinguishes this sensorial death from what constitutes actual death, that is, the cessation of all the functions, and which occurs at a later period. As far as the sensorial powers are concerned, their decline and cessation are exceedingly analogous to the approach and occurrence of sleep; the only difference being that the former is an irrevocable failure of those powers, while the latter admits of their being resumed with renovated vigour by the continued action of the vital powers.

The modes in which the sensitive functions are extinguished, or in other words *the forms of death*, are referred by the author to five different heads: the first and only natural mode is that from the simple effect of old age, when all the powers of life are completely exhausted by the continued operation of the agents which had excited them; and death is, in that case, only the last sleep. The vital functions are here impaired, chiefly from the diminished frequency of respiration, which is itself a consequence of the impaired sensibility; so that there is a diminution of the action, but not of the powers, of the vital organs. If the decay of the vital powers be gradual, and nothing occurs suddenly to accelerate it, they will necessarily cease at the time when their excitement is the smallest, that is, during the state of sleep.

In all other cases, death arises from causes which must be regarded as adventitious, and consequently inducing a more or less violent death. The first class of these causes comprises those arising from the continued action of stimulants, more powerful than the ordinary stimulants to which the system is subjected, and making their immediate impression on the organs of the sensitive system. These may be considered as producing a diseased condition of the sensorium,

which, by sympathy, communicates its influence to the vital organs. The next form of death is that which is induced by such causes as are applied, in a sufficient degree, to act as direct sedatives to the organs of the sensitive system, that is, to impair their excitability without previous excitement. The third set of causes of death comprehends those which operate by depriving some of the vital organs of those stimulants on which their functions depend; and the last consists of such as directly debilitate those organs themselves. Thus, according to the author, these adventitious causes act either directly by destroying the power of the brain and spinal cord, or by affecting the vital parts of those organs, so as, through them, to destroy the circulation or the assimilatory functions. The destruction of the circulation appears, in all cases, to be the cause of instantaneous death, and always to be effected through impressions made on the vital parts of the brain and spinal cord, except where the injurious agent operates directly on the organs of circulation themselves.

The author considers the vital functions, together with the muscular and nervous powers, which carry them on, as the results of inanimate agents acting on living parts, or living parts on them; and hence he explains the analogy which exists between all these functions and the operations of inanimate nature; while, with regard to the sensorial functions alone, as they are the results of vital parts acting on each other, so no analogy can be perceived between them and those operations.

In the course of the paper the author frequently reverts to the argument, that, to the sentient being, death being simply the loss of sensibility, the last act of dying can in no case be an act of suffering: and in the majority of instances of the long continuance of disease, our tastes, and our relish for life itself, being gradually impaired, death is met, not only with composure, but even with satisfaction.

A paper was then read, entitled, "On the Tides." By John William Lubbock, Esq., V.P. and Treasurer of the Royal Society.

Various tables relating to the tides are communicated in this paper, calculated, according to the instructions of the author, by Mr. Dessiou. In the tables given by the author in former papers, already published in the Philosophical Transactions, and having reference to the corrections due to the influence of the parallax and declination of the moon, Mr. Dessiou employed only observations of the tides made between conjunction and opposition; but in those now given, similar corrections have been obtained from observations made between opposition and conjunction.

The author enters into an inquiry into the correction due to the calendar month, which is mixed up with that due to the moon's declination, and shows that the correction for the moon's parallax, as well as declination, deduced from the theory of Bernoulli, are quite discordant with the results of Mr. Dessiou's calculations, founded on actual observation.

The author agrees with Mr. Whewell in the remark, that the theory of the tides is now in the same state as that which the theory of the

motions of the moon and planets presented about a century ago; and unless considerable exertions be made, it may so continue for many years to come. The tables of the planets have acquired their present accuracy only through the liberal encouragement of learned bodies, and of some of the governments of Europe; nor can tables of the tides, adapted to the present state of science, be now constructed, unless very considerable expense be incurred, and immense labour bestowed.

The results of numerous observations on the influence of the wind on the tides in the River Thames, are stated; and the author observes, that this is a subject of considerable importance as regards the accuracy of which tide predictions are susceptible.

The reading of a paper, entitled, "An Account of some Operations executed at Cape Frio, by the Officers and Crew of His Majesty's Ship Algerine, for the purpose of raising a part of the Stores, &c. lost in His Majesty's Ship Thetis." By the Hon. Commander F. T. de Roos, R.N., F.R.S.—was commenced.

February 27, 1834.

FRANCIS BAILY, Esq., Vice-President, in the Chair.

The Hon. Commander de Roos's paper was resumed and concluded.

The author, who had the command of His Majesty's ship Algerine, was instructed to take charge of the enterprise commenced by the officers and crew of His Majesty's ship Lightning, having for its object the recovery of the treasure and stores from the wreck of the Thetis, which, in the month of December 1830, had sunk in a cove to the south-east of Cape Frio. He reached this spot on the 6th of March, 1832, having with him eleven officers and eighty-five men. A certain number of men were appointed to remain on board the ship, which was moored in a harbour two miles off; a party of artificers and others were employed at the huts which they inhabited near the Cape; and the rest, nearly thirty-five in number, were stationed at the wreck.

The author gives a description of Cape Frio, and of the island of which it forms the south-eastern extremity, and which is an immense promontory of insulated granite jutting into the Atlantic Ocean, sixty miles east of Rio de Janeiro. The cove, in the middle of which the wreck of the Thetis lay, is a square indenture in the cliffs, six hundred feet deep by as many wide. It is surrounded by nearly perpendicular masses of granite, from one hundred to two hundred feet high, and is exposed to the whole swell of the South Atlantic, which sets in with remarkable force in that direction. The weather is singularly variable; and transitions frequently take place in the course of a few hours, from perfect stillness to the most tremendous swell. The author states that he has witnessed few scenes in nature more sublime than that presented by the Thetis Cove during a gale of wind from the south-west.

The author enters into a minute description of the mechanical apparatus employed for obtaining the necessary purchases for the various operations which were required, and gives a circumstantial history of his proceedings. Frequent interruptions were experienced from the state of the weather, and the almost incessant agitation of the water, which was often so powerful as to render the diving-bell unmanageable, and to expose the divers to serious danger. The diving-bell consisted of a one-ton ship's water-tank, with eight inches of iron riveted to the bottom in order to give it more depth, and having attached to it 18 pigs of ballast, the weight of which (17 cwt.) was found to be sufficient to sink it.

As soon as the necessary arrangements had been completed, the author states that he made a minute survey of the bottom, by means of the diving-bell, and ascertained the exact position and shape of all the large rocks which covered the spot where the treasures and stores of the Thetis had been scattered. The shape of the area where the precious metals in particular had been deposited, was an ellipse, of which the two principal axes measured 48 and 31 feet; and large boulders of granite had been subsequently rolled over these treasures, and required being removed before the latter could be recovered. The superincumbent pressure of the sea, aided by the huge materials of the wreck of the frigate, which, under the influence of the swell, acting like a paviour's hammer, with enormous momentum, had jammed together the rocks, and produced a strong cohesion between the fragments of wood, and the gold, silver and iron.

The first object was to clear away every portion of the wreck; and after this had been accomplished, to loosen and remove all the large rocks in succession, beginning with the smallest, and ending with the largest and most unwieldy. Some of these, which they succeeded in rolling from their situations into deeper water, weighed about thirty or forty tons; and the largest, which required the greatest efforts to move from its place, was computed to weigh sixty-three tons. This last effort served to show, that no part, either of the wreck or the stores, which was of any value, remained behind; and after fifteen-sixteenths of the property had been recovered, the enterprise, which had so perfectly succeeded, terminated on the 24th of July, and the Algerine returned to Rio de Janeiro on the 1st of August.

The author subjoins an account of the currents off Cape Frio, and a description of the climate, which seems to have been favourable, for his party suffered but little from sickness, and the expedition was unattended with the loss of a single life. On one occasion the party were visited by a whale, which approached very near the diving-bell, but fortunately changed its course, without doing any mischief.

A paper was then read, entitled, "An Account of a Concave Achromatic Lens, adapted to the Wired Micrometer, which has been named *Macro-micro*, from its power to increase the primary image of a Telescope without increasing the diameter of the wires in the Micrometer." By George Dollond, Esq., F.R.S.

The application of a concave achromatic lens to the wired micrometer of a telescope, arose out of the series of trials that were made for the purpose of correcting the aberrations of the eye-glasses applied to the telescope constructed by the author for the Royal Society, with a fluid-correcting lens, on the plan suggested by Professor Barlow. The concave lens, being interposed between the object-glass and the eye-glass, and being at the same time achromatic, combines the advantages of doubling the magnifying power, without a corresponding diminution of light, and without altering the apparent distances of the threads of the micrometer. The results of the trials made with telescopes to which this addition was made, are given in a letter to the author from the Rev. W. R. Dawes, of Ormskirk; from which it appears that Mr. Dollond's method was attended with complete success. Mr. Dawes states, that, in order to put its illuminating power to a severe test, he had examined with this instrument the satellites of Saturn and the minute companion of κ Geminorum, but could discover no decided difference in the apparent brightness of the former, allowance being made for the difference in the power employed; and the latter star was seen quite as distinctly with a much smaller power.

Extracts are subjoined from a letter of Professor Barlow's to the author, containing formulæ for the construction of the lens.

March 6th, 1833.

MARK ISAMBARD BRUNEL, Esq., Vice-President, in the Chair.

The reading of a paper, entitled, "On the Structure and Functions of tubular and cellular Polypi, and of Ascidiæ." By Joseph Jackson Lister, Esq., F.R.S.—was commenced.

March 13th, 1833.

JOHN WILLIAM LUBBOCK, Esq. M.A., V.P. and Treasurer, in the Chair.

The reading of Mr. Lister's paper was resumed and concluded.

This paper contains the account of a great number of observations made by the author during the last summer, while he was at the southern coast of England, on several species of *Sertulariæ*, *Plumulariæ*, *Tubulariæ*, *Campanulariæ*, *Flustræ*, and other polypiferous zoophytes, and also on various *Ascidiæ*. Each specimen was placed for examination in a glass trough with parallel sides, before the large achromatic microscope of the author, directed horizontally; and care was taken to change the sea-water frequently, which was done by means of two syphons, the one supplying fresh water, while the other carried off the old; a plan which succeeded in keeping the animals in perfect health and vigour. The drawings which were taken of the

appearances that presented themselves were traced with a camera-lucida, slid over the eye-piece of the microscope.

In a specimen of the *Tubularia indivisa*, when magnified 100 times, a current of particles was seen within the tube, strikingly resembling, in the steadiness and continuity of its stream, the vegetable circulation in the *Chara*. Its general course was parallel to the slightly spiral lines of irregular spots on the tube; on one side flowing from, and on the other towards, the polypus, each current occupying one half of the circumference of the tube. The particles were of various sizes, some very small, others larger, but apparently aggregations of the smaller: a few were nearly globular, but in general they had no regular shape. At the knots, or contracted parts of the tube, slight vortices were observed in the current; and at the ends of the tube the particles were seen to turn round, and pass over to the other side. Singular fluctuations were also observed in the size of the stomach and of the cavity of the mouth; the one occasionally enlarging, while the other contracted, as if produced by the passage of a fluid from the one into the other and its subsequent recession, thus distending each alternately. This flux and reflux took place regularly at intervals of 80 seconds; besides which two currents were continually flowing, both in the mouth and stomach; an outer one in one direction, and an inner one in the opposite direction.

In all the species of *Sertulariæ* examined by the author, currents of particles were observed passing along the soft substance which occupies the axis of the stem and branches, and were even seen extending into the substance of the polypi themselves, and traversing the stomachs belonging to each. Contrary to what happens in the *Tubularia*, the stream does not, in these animals, flow in the same constant direction; but after moving towards one part for about a minute or two with considerable velocity, it becomes much slower, and then either stops or exhibits irregular eddies, after which it resumes its motion with the same velocity as before, but in the contrary direction; and so on alternately, like the ebb and flow of the tide. If the current be designedly obstructed in any part of the stem, those in the branches go on without interruption, and independently of the rest. It appears from a passage which the author has quoted from Cavolini, that he had noticed the circumstance of currents existing in the interior of *Sertulariæ*, but had not detected their continuation into the stomachs of the expanded polypi. Similar phenomena, which the author describes in detail, were observed in several *Campanulariæ* and *Plumulariæ*; and several particulars are noticed with regard to the ovaries, and to the movements of the fluids contained in the ova of these zoophytes, before their exclusion from the body of the parent. In some cases, the young polype, after it has attained a certain growth, but while still adhering to the parent, becomes decomposed, and, its substance being absorbed into the body of the latter, it entirely disappears. Changes of the same kind frequently take place in different parts of the whole group; one of the polypes being seen to shrink and gradually disappear, while others shoot forth in more luxuriant growth, rapidly acquiring a large size. The author regards the

circulating fluids in these animals as the great agent both in the absorption and the growth of parts, and throws out the suggestion, that as it flows into the stomach, it may also act as a solvent to the food received into that cavity. The particles which exist in these fluids show their analogy to those in the blood of the higher animals on the one hand, and to those in the sap of vegetables on the other: some appear to be derived from the digested food, and others from the melting down of parts absorbed. In these polypi the author never saw the least appearance of cilia, or of currents in the surrounding water, which are so frequently met with in other tribes of zoophytes.

The latter part of the paper is occupied by the account which the author gives of his various observations, first, on *Ascidiæ*, of which he enters into an anatomical description; secondly, on the internal currents of water, permeating the branchial sacs, and determined by the vibratory movements of cilia which are seen in that animal; and, thirdly, on the alternations in the course of the circulation of the blood in the vessels, which at one time flows in one direction, and, after a certain interval, takes the contrary course; so that the same vessel which at one time performs the function of an artery, performs, at another, that of a vein. This phenomenon of alternate currents, like that in the *Sertularia*, was met with in every specimen of *Ascidiæ* which was examined by the author, and also in a *Polyclinium*.

The paper concludes with several observations on *Flustræ*, from which, as far as relates to the circulation of currents, the author was led to results in many respects analogous to the preceding.

A paper was then read, entitled, "On the Theory of the Moon." By J. W. Lubbock, Esq., V.P. and Treasurer of the Royal Society.

The author, adverting to the appearance of M. Plana's admirable work entitled *Théorie du Mouvement de la Lune*, enters into a comparison of the analytical methods employed by that author and M. Damoiseau, and points out some differences in the numerical values of the coefficients of some of the arguments in the expression for the true longitude of the moon in terms of her mean longitude. He then prosecutes the subject by a series of analytical investigations, which are not susceptible of abridgement, but from which he obtains formulæ which do not quite agree with those of M. Plana.

A paper was also read, entitled, "Some Suggestions relative to the best method of employing the new Zenith Telescope lately erected at the Royal Observatory." By John Pond, Esq., F.R.S., Astronomer Royal.

During the observations made by the author, in the course of last summer, with the new zenith telescope lately erected at the Royal Observatory, for the purpose of measuring the zenith distance of γ Draconis, it occurred to the author to avail himself of subsidiary observations on another star, of about the fifth magnitude, which has nearly the same zenith distance towards the south that γ Draconis has towards the north, and which passes the meridian between 20 and 30 minutes, in time, after it. The angular distance between the

two stars being determined in the usual manner, by observing them on the same night, and in the same position of the instrument, gives the *sum* of their zenith distances : and if on the next or some following night γ Draconis be observed, and after its passage the instrument be turned half round, and the other star observed, then the difference of the measure, as read on the micrometer, will be the *difference* of the zenith distances of the two stars. These sums and differences, thus ascertained on different nights, will be independent of any change that may happen to the instrument in the interval. This method affords the means of determining, with almost unlimited precision, the value of the small equations which become the subject of investigation in the employment of the instrument. Thus all changes of the position of the stars, occasioned by aberration, nutation, &c., will produce double the effect on the small differential or subsidiary angles, as measured by this method. For the investigations of these small equations it will not be necessary to have determined either the exact zenith distance of each star, or the exact difference of their zenith distances, or the absolute magnitude of this subsidiary angle; its variation from time to time being the only important object of research. The author is led to expect that this property may, at some future period, be applied with advantage in investigations made with moveable zenith instruments.

March 20th, 1833.

MARK ISAMBARD BRUNEL, Esq., Vice-President, in the Chair.

A paper was read, entitled, "Narrative of the Proceedings of Commander Thomas Dickinson, of His Majesty's Sloop Lightning, while employed in the Enterprise for the Recovery of the Public Stores and other property sunk in His Majesty's late Frigate Thetis, on the south-west side of the Island of Cape Frio." By Commander Thomas Dickinson, R.N. Communicated by P. M. Roget, M.D., and J. G. Children, Esq., Secretaries to the Royal Society. It was preceded by the reading of a letter from the author to the Secretary, explaining the reasons which induced him to lay this narrative before the Royal Society, and place on the records of its proceedings the information it contains relative to the commencement of an enterprise, wholly planned and undertaken by himself, and which, under his superintendence, was, by the great, persevering and meritorious exertions of his officers and crew, most successfully accomplished.

The narrative commences with the statement of the consternation produced at Rio de Janeiro on the receipt of the intelligence of the loss of the Thetis, with a freight of about 810,000 dollars, on the south-west side of the island of Cape Frio, and of the determination of the author, on finding that no one seemed disposed to take any step towards the recovery of the property thus lost, to make the attempt himself, if he could obtain from the Commander-in-chief at that

station, Rear-Admiral Thomas Baker, C.B., orders to that effect. He accordingly exerted himself to obtain every possible information relative to the nature of the coast, depth of water, and other circumstances, which might enable him to judge of the practicability of the undertaking, and of the means necessary for its successful accomplishment; and became convinced that the difficulties and obstacles to be encountered, although numerous and formidable, might be overcome by the employment of the means which suggested themselves to him as practicable on this occasion, if sufficient assistance were afforded him in putting them into execution. He accordingly had models of the proposed machinery made, and submitted them, together with his plans, to the Commander-in-chief, by whom they were approved. He experienced great difficulties in procuring a suitable diving-bell, for it was impossible to obtain any instrument of the kind at Rio de Janeiro, or even any facilities for the construction of one by casting. It at length occurred to him that a ship's iron water-tank might be converted to this use; and being supplied with one from the Warspite, he was enabled to render it available for that purpose. The next difficulty was to procure an air-pump, which, after much delay, owing to the tardiness of the native workmen in that country, was at length constructed. The want of air-hoses, however, was a still more formidable obstacle to the success of the plan; but the ingenious contrivances of the author for rendering the common pump hoses air-tight, supplied this deficiency; and on a trial which he made with the whole apparatus on the 22nd of January, 1831, it was found to answer completely. The next day he received his orders from the Commander-in-chief, and, sailing on the following day, arrived at the harbour of Cape Frio on the 30th, and immediately proceeded to inspect the coast, and ascertain the situation of the wreck, not a vestige of which was visible. An account is then given of the local circumstances of the Thetis Cove, or inlet, surrounded by almost perpendicular cliffs from 108 to 194 feet in height, with a depth of water varying from $3\frac{1}{2}$ to 24 fathoms, and the bottom being strewed with huge perpendicular rocks, occasioning these inequalities. These surveys showed that the execution of the plan originally conceived by the author was opposed by so many unforeseen difficulties, that he was obliged to relinquish some parts of it, and resort to fresh expedients for surmounting them. The idea of constructing a derrick then occurred to him; but the materials were wanting, for no trees existed in the island except those in the forests in the interior, which were inaccessible from their distance and the heights on which they grew, and of which the wood was, from its quality, unsuitable to the purpose. His only resource, therefore, was to make it of the fragments of spars saved from the wreck. With great exertions, a circumstantial account of which is given in the paper, the work was at length accomplished; and the result fully equalled the anticipations which had been formed of its utility in affording a stable point of support for the operations with the diving-bell. Previously to the erection of a derrick, however, much had been done by working the diving-bell from a boat, and a considerable quantity of stores and treasure raised. At one time the anxiety

of the author to forward the undertaking, and avail himself of favourable weather, induced him to try the experiment of working by torch-light, which succeeded to a certain extent; but after a few trials the danger was found to be excessive, and the fatigue to the divers so great as to oblige him to desist.

After the derrick had been for some time in operation, a tremendous sea arose, the shock of which, for want of sufficient materials to support it, effected its destruction; and a substitute was then resorted to by the setting up of a suspension cable diagonally from the cliffs, which, after great difficulties, was at length effected.

A great portion of the narrative is occupied with the details of the various proceedings, and of the serious impediments which were successively overcome by the zeal, perseverance and extraordinary exertions of the officers and crew, under the orders of Captain Dickinson, subjected as they were, for so long a period, to the greatest privations and hardships, arising from the laborious nature of the work, the unhealthiness of the climate, the attacks of the chigger, producing distressing ulcers in the feet, the annoyance from drifting sand, which penetrated into every place, the exposure to constant wet in huts which could not be made to exclude either wind or rain, and the perils arising from the boisterous gales and tremendous swell of the sea, which the whole ship's company, but more particularly the men in the diving-bell, had to encounter; forming a combination of difficulties which the author is convinced could have been surmounted by none but British seamen.

After having succeeded so far in the undertaking, and made, at various times, shipments for England of treasure amounting to about three fourths of the whole which had been on board the Thetis when she sunk, orders were received by the author, on the 6th of March, to resign the charge of the enterprise to the Hon. Capt. De Roos, of His Majesty's brig Algerine; on the receipt of which he immediately ordered a survey to be taken of the stores, and on the 9th descended in the bell, surveyed the bottom of the Cove, ascertained the position of the remaining stores and a considerable quantity of treasure; and after having communicated the whole of the results to Captain de Roos, instructed him, his officers and ship's company in the way of working the bell, as well as in the different modes of removing rocks, recovering stores and treasure, and the use of the whole of the machinery, and furnished him with every necessary information for his guidance, he lent twenty of his men to the Algerine for their assistance, resigned the charge to his direction on the 10th, and sailed for Rio de Janeiro on the 13th.

Annexed to the paper is a journal of the amount of treasure of various descriptions recovered between the 31st of March, 1831, and the 10th of March, 1832, by His Majesty's sloop Lightning.

There was then read an extract of the letter of instructions, bearing date the 10th of March, 1832, from Commander Thomas Dickinson, then of His Majesty's ship Lightning, to Commander the Hon. S. F. de Roos, then of His Majesty's brig Algerine, on the former re-

signing to the latter the charge and direction of the enterprise for the recovery of the public stores and treasures sunk in His Majesty's late frigate Thetis, off Cape Frio.

The Society then adjourned over the Easter Recess, to meet again on the 10th of April.